"Light" (Chas. Scribner's Sons, New York, 1901), where pages 139 to 153, inclusive, are given to a popular discussion of halos, and in Appendix C, [is given] a more rigid treatment with criticism of Bravais's views together with a substituted theory, which, in my mind, is acceptable. In this I succeeded in explaining all of the authenticated phenomena of halos, 14 in all, with the exception of the famous 90°-circle of Hevelius which would be the 15th. At that time I was almost disposed to question the reality of this feature; now, my attitude is somewhat changed, and I am inclined to a belief that it admits of theoretical explanation. This explanation I have not published because certain of its assumptions are not sufficiently based upon observation. * * *

"The first and most voluminous writer upon the subject, and perhaps the most philosophical, was Bravais. Bravais's fundamental errors were the following: (1) an error in mechanics contained in the assumption that elongated crystals would fall through the air with their axes vertical and plate-form crystals with their axes horizontal; (2) that ordinary reflections from the faces of such crystals could produce anywhere the notable increase in sky luminosity which characterizes the features of halos; (3) that he was justified in assuming the presence of any desired form of ice crystals convenient for his purposes provided that they did not contradict the laws of crystalography, overlooking, moreover, the fact that in order to attain his explanation he must assume the great predominence of that particular type in just the desired direction.

"Writers who have followed Bravais have, to the best of my knowledge, corrected only the first of these fundamental errors, namely, the mechanical ones. The optical and the crystalographical have not been touched, see figures 12, 13, 14, of page 434 of the MONTHLY WEATHER REVIEW for July, 1914.

"Let us consider briefly the significance of the three criticisms above, or rather, since everyone agrees as regards the first, let us turn our attention to the others. When we regard an ordinary feature of the halos, the 22°-ring for example, the origin of which is explained to the satisfaction of everyone, it will be observed that very nearly all of the light which enters a face of a suitably oriented crystal emerges in the direction of the observer other crystals present merely diluting the phenomenon. Now imagine the amount of light sent from such oriented prisms reduced to one-twentieth or less, can anyone suppose that under such circumstances any very marked or even notable increase of luminosity could be found in this region? But this is just the ratio of the decrease of luminosity when one depends, as do all of these writers, upon ordinary reflections from the crystals.

"As to the third criticism, it hardly needs more than a statement to render it valid. That Bravais should have premised a large number of unknown crystal forms merely because he thought they would meet his theoretical requirements is not so surprising; but that anyone else should invent a host of new forms which have an even less probable actuality is certainly very surprising. The tremendous outstanding objection to this method, which appears above, has never been touched upon as far as I know—I mean, that even granting the existence and efficiency of those highly complicated crystals one must put them in enormous numerical majority in just the required direction in order to be effective.

"The principles at the base of my theory are also three, and of the simplest kind: (1) Only such forms of ice crystals as have been observed and are of very simple type can be presupposed. This is an almost self-evident condition since the phenomena necessarily infer an exceptional homogeniety of forms, for otherwise since prescribed forms only are effective, the presence of all others would only add to the whiteness and opacity of the sky; (2) the orientation of the crystals in falling must obey the law of mechanics; (3) all of those features of halos which are attributable to reflections must find their explanation in every case in total reflections.

With these narrow restrictions, made by no other writer, I had [in 1900] succeeded in explaining all well authenticated phenomena of this class with the postulate of only 2 forms of perfectly well-known crystals."

SYSTEMATIC EXPLORATIONS OF THE UPPER AIR WITH ESTIMATES OF COST.[1]

By MARK W. HARRINGTON, Chief, U. S. Weather Bureau.

[Read before the International Conference on Aerial Navigation, Chicago, Ill., August, 1893.]

[This paper is of such historical value that we reprint it in full from the original for the information of students of meteorology. The paper was briefly referred to in the Monthly Weather Review, June, 1897, **25:** 313, and January, 1914, **42:** 39.

Many will be interested to learn that Prof. M. W. Harrington is still quietly living near Philadelphia, Pa.—C. A.]

The exploration of the upper air is the immediate requirement for the satisfactory advance of meteorology. There is abundant reason to think that many of the changes which go under the name of weather have their origin at some distance above the earth; and of what occurs in the cloud layer or layers, our knowledge is insignificant or theoretical. The only systematic attempt to investigate the higher atmosphere has been by means of mountain stations; but this, though it has led to a series of interesting results, does not meet the requirements of the meteorologist. The station on the mountain top is after all only a station on the earth's surface; and though many of the equidynamical surfaces show change with the elevation of the land (the isobaric, for instance), others (as the isothermic and those for wind and humidity) show marked adaptation to the contour of the surface. Many aeronauts have noted this adaptation as especially true of the cloud layers, the lower one often reproducing with some exactness the general variation of the surface below. We can hardly expect, therefore, that the mountain stations, useful as they are, will give us the aid needed in ascertaining what goes on at considerable elevations in the free air.

There are several ways of exploring the upper air by investigating the ray of light which has passed through it. The spectroscope promises much in this direction. The twinkling of the stars might be expected to give us a great deal of information when properly interpreted; Señor Ventosa has shown that even the fluctuations on the margins of the larger celestial bodies, when viewed in the telescope, have apparent relations with the upper winds. This information must, however, be vague, because the total result received by us is the integration of the individual effects at each point of the path, and it is not practicable to separate the sum into its parts. Besides, even if this could be done, the information to be obtained would be very incomplete, as it would relate only to a part of the series of meteorological elements. It may be mentioned as of interest in this connection that the scintillation of stars has been especially and systemati-

[1] *Reprinted from* pp. 349–354 of Proceedings, International conference on aerial navigation, Chicago, Aug. 1, 2, 3, and 4, 1893. Amer. eng. and railr'd jour. New York, 1894, iv, 429 p. 8°.

cally studied; and M. Dufour, one of the leading students of the subject, has recently announced (Archives des Sciences Phys. et Nat., June, 1893) that the only meteorological result he has been able to reach is the rule that lessened twinkling indicates bad weather.

There remain as means of systematic exploration of the free air, elevated towers, kites, pilot balloons (without aeronauts), and balloons carrying aeronauts. The elevated towers are well illustrated by the Eiffel Tower in Paris. By such a tower a systematic study may be made of a layer of air 1,000 feet thick, with almost infinitesimal perturbations by the tower itself. The excellent series of observations made by the French National Service on the Eiffel Tower have proved of very great interest, yet they do not reach to the height needed for the study of the upper air. It tells us nothing of what happens in the cloud layer, probably the most important of the strata of the atmosphere. Moreover, such towers are very expensive to build and to maintain. I have heard the cost of the Eiffel Tower estimated at $1,000,000, and its maintenance must cost a considerable sum, which could only be met by using the tower as a permanent show place; the latter requirement necessitates its being placed in or near some great city.

The method by kites has been studied especially by Mr. William A. Eddy, of Bergen Point, N. J., and the data which I give I owe entirely to his kindness. He uses tailless kites, places them in tandem, and recommends that they be flown in groups of three. By such means he has already attained heights of 4,000 to 5,000 feet, and confidently expects to attain 14,000 feet without serious difficulty. On my request that he estimate the cost of carrying meteorological instruments to this height, he gave me the following estimate, on the basis that the line would average an angle of 45° with the horizon, and would have to be about 23,000 feet in length.

Cost to carry instruments to 15,000 feet height by means of kites.

			Breaking strain.	Cost.
			Pounds	
8 highest kites	12,000 ft.	Cable-laid twine	250	$6.00
8 next kites	3,000 ft.	$\frac{3}{16}$-inch rope	540	6.00
8 next kites	3,000 ft.	$\frac{3}{8}$-inch rope	1,280	12.20
9 next kites	5,000 ft.	$\frac{1}{2}$-inch rope	2,250	30.50
Sum	23,000 ft.			54.70
KITES	8 kites 5 feet across			$16.00
	8 kites 6 feet across			20.00
	8 kitse 8 feet across			24.00
	9 kites 9 feet across			31.00
Sum	33 kites			91.00
Windlass for winding kites				$20.00
4 laborers, at $1.50 per diem				6.00
				26.00
Total				171.70

Mr. Eddy adds "that in lighter winds perhaps 50 kites would be required, the above estimate applying for winds of about 10 miles per hour. All the kites are tailless, and fly at an angle of about 80° from the horizontal for the first 300 feet of line out. In case the pull becomes too great for the breaking strain, the low and larger tandem kites can be hauled in. The breaking strain of the cordage must be known and the pull at the earth's surface constantly measured to prevent the entire line from breaking away. This is a rough estimate, but is founded upon careful experiments during two years. The top kites and twines should be laid out the night or day before and the lines should be extended along the ground for several thousand feet. Soon after daybreak the top kites should be started up, the top one lifting the next, and so on. The kites will right themselves, regardless of position in which they are when lifted by the higher kites. Instruments should be suspended between 2 groups of 3 kites each, thus (see figure omitted).

"Three tailless kites will fly when any one of the three will not, in very mild surface winds. For safety it would be well to have the kites in groups of threes."

Mr. Eddy is not ready to give a limit to which kites can be flown, but is not without hope that they can be made to reach the cirrus clouds. In winds of high velocity the kites must be perforated to relieve them fron too strong air pressure. The tailless kites easily right themselves when reversed, and a tandem series of kites tends to prevent the jerking which might put the instruments out of order.

It seems, therefore, that for about $200 we may hope by means of kites to take instruments for registry to the height of about 14,000 feet. If we assume a loss and wear of 5 per cent per day in the kites and other apparatus, we would have a current expense of $10 per day. Add to this the $6 for labor, and such service would cost about $16 per day. This would make for current expenses for a year $5,840; add cost of instruments, $2,500; outfit and incidentals, $1,660; total, $10,000.

A probable cost, then, of $10,000 for a year's systematic work of this sort, not including the salary of the official in charge.

PILOT BALLOONS.

The best possible anemometer is a balloon which is immersed in the air and moves freely with it. For half a century or more occasional studies of the lower air currents have been made by means of small pilot balloons. The balloon is allowed to rise freely, and a card is attached to be returned by the finder with name, date, and place. The most elaborate series of observations of this sort known to me are those of M. Louis Bonvallet at Amiens, who, from May, 1888, to the end of 1890, sent up 97 paper balloons varying in volume from 50 to 1,800 liters. The general results are given by M. Gaston Tissandier in La Nature (Paris), 1891–92, pages 259–260. The amount of instruction from them is small and disappointing.

Such balloons can be used only for the study of air currents, but by a proper selection of places and dates and the assistance temporarily of theodolites and persons capable of using them these balloons could be made very useful. They would enable us to study the arrangement of air currents about definite meteorological phenomena, such as centers of high or low pressure. To effect this the observer should have a supply of small balloons on hand and the means of readily inflating them. He should also have on ready call two theodolites and persons capable of using them—a requirement easily filled at any college or university with a department of civil engineering. The weather map should be carefully studied from day to day, and when a "high" or "low" is about to pass over the station the force of observers should be called out and the balloons inflated and released to as great a number as could be observed at frequent intervals (once in 5 minutes, say), and with approximate precision. Computation would then easily show the horizontal motion at determinate elevations below the cloud layer, and plotting would show the relation of these to the center of pressure. Aside from the salary of the principal observer, the cost of such observations would be small for each opportunity to observe, and for any given station the number of opportunities during the year would be few. The expense at any station for a year would probably not surpass $150, so that for $3,000 such observations could be scattered at 20 colleges over the States, with probable results far in excess in value over the cost.

A more instructive but more expensive method is that of pilot balloons carrying automatic registering instruments. This method of sounding the upper air was proposed by Le Verrier in 1784, and has within the last few years been repeatedly tried in France. In the last four months of 1892 M. Hermite sent up 13 such balloons, all of which reached an altitude of over 9 kilometers, or 6½ miles; and one sent up on March 21, 1893, must have reached an elevation of over 16 kilometers, or 10 miles. These balloons carry means for the automatic record of pressure and temperature, but the last-mentioned found so cold temperatures that for a considerable time the specially prepared ink could not perform its functions. They also carried a device for releasing and dropping cards, to enable the following of the course of the balloon; but this has not been successful, as the fuse which releases the cords is soon extinguished. In the ascent of March 21, out of 600 cards taken up only 400 were released, and of these only 5 or 6 were recovered. It is found, however, that the recovery of the balloon is much easier than had been expected, as a printed direction on the balloon itself leads to its recovery as soon as it falls into the hands of any intelligent person.

The difficulties in the way of these remarkably interesting explorations prove to be less than could have been expected; but there are many questions about them still unsettled. Under these circumstances it is not easy to make an estimate of the cost of systematic work in this direction. I have, however, asked Prof. H. A. Hazen to make the estimates for me. He has estimated approximately that a balloon to ascend to a height of 4 miles with a load of instruments of 20 pounds would cost $150 if made of silk and $200 if made of goldbeater's skin. For a balloon to ascend to the height of 10 miles he puts the corresponding prices at $600 and $800. The instrumental outfit would have to be prepared expressly and would be expensive. Probably the sum of $5,000 would permit of one such pilot balloon per week during the year to the height of 4 miles, and perhaps one per month to the greater height. The station selected for such observation should be near the middle of the continental area—say somewhere from Kansas to Manitoba.

BALLOONS WITH AERONAUTS.

The preceding methods, while they would give highly interesting and instructive results, are somewhat imperfect as means of obtaining all the information needed by meteorologists. Much better for this purpose would be systematic work by a meteorologist who should make the ascension himself. Evidence points to the conclusion that the cloud layer, and perhaps the upper cloud surface, is a region of especial activity in meteorological phenomena, but the facts on which such a conclusion could be verified are of such character that they would probably escape any automatic registry. Their record requires the presence of a trained meteorologist. These observations should be systematic, as the sporadic ones are of relatively little value. A meteorologist should ascend twice a day to a considerable height, and should keep this up through all kinds of weather and through the season. The elevation need not be great; probably the first 20,000 feet include the layer of air in which the meteorological phenomena which we call weather are active. At least the stratum of this thickness is far more important to us than all the rest of the depth of the atmosphere.

The cost of such a campaign would be considerable, but would vary with the material used, the care in using it, the position of the station, etc. I think a year's campaign of this sort could be gone through for an expense of $20,000.

In conclusion, it appears that a year's campaign could be made in the free air as follows:

To 3,000 feet (perhaps), with small balloons	$3,000
To 14,000 feet, with kites	10,000
To 20,000 feet, 52 pilot balloons	} 3,000
To 50,000 feet, 12 pilot balloons	}
To 20,000 feet, with aeronaut	20,000

The results to be obtained would be cheap at any of these prices, but the fourth method seems to me incomparably the best as well as the most certain. A year's campaign of this sort would add very greatly—more than in any other possible way in the same time—to the knowledge of meteorology and hence to the forecasting of the weather. There is no other way, I believe, in which this sum of money could be expended to the greater advantage of meteorology.

(NOTE.—Upon the reading of the above paper it was, upon motion of Mr. D. Torrey, unanimously resolved [by the Conference]:

"That it is the sense of this meeting that the experiments proposed by Mr. Harrington are likely to prove of public value in forecasting the weather, and that Congress should, in our judgment, make the necessary appropriation to have the experiments made as recommended by Mr. Harrington.")

EXTRACTS FROM THE ANNUAL REPORT OF THE SMITHSONIAN ASTROPHYSICAL OBSERVATORY.[1]

C. G. ABBOT, Director.

[Dated: Washington, D. C., June 30, 1914.]

Observations [*at Washington*].—Mr. Fowle has continued the difficult research on the transmission through moist air of radiations of great wave length, such, for instance, as those which bodies at the temperature of the earth emit most freely. He uses a very powerful lamp made up of a large number of Nernst electric glowers, and examines by the aid of the spectrobolometer the energy spectrum of the rays emitted by this lamp, first directly, and then, after the rays have traversed twice or four times a tube 200 feet long, containing air of measured humidity. During the past year Mr. Fowle has been dealing principally with rays of the very longest wave lengths of the terrestrial energy spectrum which moist air transmits. He has reached a wave length of about 18 microns, which is about 25 times the longest wave length visible to the eye, and about three and one-half times the wave length of the solar rays investigated by this observatory in the years 1890 and 1900.

A great number of difficulties are met with. In the first place, great sensitiveness of the bolometer is required, owing to the feebleness of these rays. Attempts to use a vacuum bolometer have consumed much time, but not yet with entire success. Full success in this

[1] *Extracted from* Appendix 5 of Smithsonian Institution. Report of the Secretary for the year ending June 30, 1914. Washington, 1914. iii, 117 p. 4 pl. 1 fig. 8° (Publication 2317.)